THE VETERINARY RECORD

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Class Honours.

Last week we put a note at the foot of the list of successful candidates at the Examinations in Scotland. This note, whilst attempting to do justice to one gentleman, might possibly be read as doing injustice to another. The statement that a London student had obtained honours in the final examination was followed by this comment—"a distinction which has not been gained by anyone in the final examination for the past three years." As a fact the distinction was gained by a Glasgow student at the recent examination and an asterisk against the name of Mr. H. G. Simpson denoted his position. Our comment was of course not dictated by any feeling for or against individual candidates, but was suggested by a remark in one of the inaugural addresses delivered at the opening of the session. The lecturer then pointed out the remarkable circumstance that whereas men in classes A and B obtained a very creditable and even high proportion of honours, they all failed to reach an honours grade in the C examination. We do not propose now to discuss why the higher standard is not reached in the final. It was worthy of note that the record of three years had been broken. There are only two possible explanations of the lower marks obtained in the examinations on medicine and surgery—one, that students are defective in their practical clinical knowledge; the other that the examiners put their standard of requirements too high. Probably teachers and practitioners would accept opposite explanations, but it may be that there is some truth in both. Let us hope, now a new departure has been made, that at next examination an even better position may be obtained by candidates in class C, and that a large proportion may reach honours.

The Pleuro-Pneumonia Outbreak.

The case of pleuro-pneumonia discovered by Mr. Shaw, F.R.C.V.S., has been followed by slaughter of the fifty animals forming the herd in an East-end cowshed. Three more cases of disease have been found on post-mortem examination, so that altogether four were infected. Two of these were old cases of disease, with recent extension, two were recent cases which had no doubt become infected from the others. The four diseased cows were part of a lot of five purchased from a dealer on November 4th last, and there is a rumour that the origin of the disease may possibly be traced to Ireland.

If this outbreak had not been thus cut short at its commencement, the owner of the cows might have quite innocently sold infected animals and spread disease to many fresh centres. It is not a difficult matter for the Board of Agriculture to send an officer to superintend the slaughter of all the cattle on a farm or in a cowshed when the disease has once been found. In this case, the discovery was due to a veterinary surgeon who, though an inspector for the local authority, had not been called officially to the place. How many outbreaks of contagious disease are discovered by veterinarians? Nearly all. The nature of their calling necessarily brings them on the scene when disease is suspected. But their trained intelligence often enables them to detect disease in a market or fair, in a cowshed or a butcher's shop when no suspicion of the danger has entered the mind of the owners. We know a number of cases in which the voluntary action of a practitioner has thus been the cause of detecting a diseased herd or flock. At the meeting of the National Veterinary Association more than one instance was related of how a veterinarian passing through a market had detected swine-fever in unsuspected pigs.

Our object in mentioning these facts is not to obtain credit for altruistic benevolence. The profession is quite willing to assist in controlling animal diseases, but it desires acknowledgement of its work. It denies that soldiers and sailors can take its place. It claims that central and local authorities should cease to ignore its unique position. The value placed upon veterinarians by the Board of Agriculture is too often accepted as just by local authorities, and thus the public estimate of the profession is debased. We cannot expect another invasion of cattle-plague to forcibly demonstrate our utility. Without a national panic we expect little recognition from authorities, save what we can compel by the strength of our position and the constant use of our privileges.

The Northallerton County Council has just passed a resolution exhibiting the most child-like ignorance. One might have expected that the members of an elected council in a little provincial town in Yorkshire would have known something of animal diseases and their spread, but this is what they say—"That the compulsory attendance of a veterinary inspector at every market and fair is of no value and causes unnecessary expense." And this, too, during the prevalence of swine-fever.

The fact is that centralisation is demoralising local communities. They do not pay for the suppression of disease and are not entrusted with power to do the work. They note that a central department ignores the veterinarian and they follow the example. Why should we not reconsider our position? We want a *quid pro quo* and a little organisation on our part would compel it.

SPLINTS.

By R. C. Irving, F.R.C.V.S.

Although splints are an unsoundness they seldom are a cause of more than temporary idleness in horses. They are, however, a source of frequent annoyance to the veterinary surgeon. As a rule the lameness resulting from them yields to a small blister and a week or two's rest. But not always, and it is the exception which causes trouble. One of the first questions a client asks when a splint lameness is under consideration for treatment is—"how long will he be before he is again fit for work?" A probable prognosis suggests one month, and the practitioner cautiously replies "about a month." In the majority of cases he is right, but in a few the history is far from satisfactory; something like this—blister applied, lameness disappears, leg fines down and exercise is commenced. Work is resumed at the end of the month, and the second journey results in reappearance of lameness. Explanations are offered and treatment again adopted. Previous history repeats itself, lameness departs, work is resumed and again lameness results. Further explanations are offered, and this time the splint is puncture-fired as well as blistered. Slow exercise is cautiously adopted and as much time as possible obtained before work is permitted. Then perhaps, but not always, the horse remains sound; but the client has received a shock to his confidence by a loss of the horse's services extending over three months instead of the one first prognosticated.

I am not referring to bad splints, such as interfere with the knee joint or the tendons, but to every day exostoses which seem to all appearances the same as we see develope in some horses without causing a single day's loss of work. I may be wrong, but I know no diagnostic signs by which an opinion can be guarded against such mistakes in prognosis.

Then again splints cause a practitioner trouble sometimes in connection with examination for soundness. They should always be mentioned in a certificate when found, or serious consequences may follow neglect. When mentioned, a simple splint in a horse over five years old, the qualification may be added "which, in my opinion, will not interfere with his usefulness." This, to a client who understands horses, does no harm and it protects the practitioner. In some cases it frightens an ignorant buyer and stops the sale of a good horse, but of course such possibilities must not be allowed to guide us in framing a legal document like a certificate of soundness.

I remember a case which occurred some time ago to a brother practitioner, who, very unfairly, lost his client. He examined a five-year-old horse and certified it sound, mentioning a splint on the near fore leg which he characterised as not likely to be any detriment to the horse. He went further than this to protect his client and obtained a special warranty for the splint for one month, much to the disgust of the dealer. The very first journey performed by the newly purchased horse resulted in his coming home lame on the off fore. In the course of a few days it was evident that a splint under the knee was the cause, and nearly six months elapsed before work was resumed with soundness.

I still consider splints a trivial defect, but we may undervalue them as practitioners, and it is well always to be cautious in giving an opinion as to their significance and as to the length of time required to cure lameness resulting from them. They are essentially objects which demand tact as well as knowledge to enable one to steer a clear course between pathological pedantry on the one side and practical assurance on the other.

CANCER-ROOT, OR BEECH-DROPS.

(*Epiphegus Virginiana.*)

Dear Sir,

As there has been a discussion of late in your journal respecting a cancer cure, I have forwarded you the following as I thought it might be of some interest to your readers who might like to know of other cancer cures.—Yours faithfully,

Quack.

A parasitic plant of the natural order Orobancheæ (q.v.), a native of North America, growing almost exclusively on the exposed roots of beech trees. Like all the other plants of its order, it has a curious appearance, having scales instead of leaves. Its stem is branching, and produces distant alternate white flowers streaked with purple. The whole plant is powerfully astringent, and the root is brownish, spongy, and very bitter and nauseous in taste. It has acquired, in its native country, the reputation of being a cure for cancer. All parts of the plant are used, and externally more than internally. This plant, in conjunction with white oxide of arsenic, is believed to have formed a medicine once famous in North America under the name of Martin's Cancer-powder. Another American plant of the same order, *Phelipæa biflora*, is sometimes also called cancer-root, and is used in the same way; and an infusion of the *Common Broomrape* (Orobanche major)—a native of Britain and the South of Europe, parasitic on the roots of broom, furze, and other leguminous plants—has been employed as a detergent application to foul sores.—(*Chambers' Encyclopædia of Universal Knowledge.*)

PSITTACOSIS, OR THE INFECTIOUS DISEASE OF PARROQUETS.

The point in question, we understand, is that an infectious disease of a special type is transmitted to man by parroquets or parrots affected with the same complaint. MM. Gilbert and Fournier have just reported the result of their bacteriological researches which they undertook during the month of February last, when all the members of a family in Paris became attacked with Psittacosis.

It is known that M. Nocard described, in 1893, a baccillus which he found in the medulla of the bones

of the wings of parroquets brought to France from America. MM. Gilbert and Fournier have also found this bacillus in the viscera and spinal cord of a parroquet which had succumbed, and in the blood in the heart of a woman dead of Psittacosis.

It is extremely virulent not only for parroquets but also for different other animals, as the mouse, guinea pig, rabbit and pigeon.

This baccillus by some of its characters is not unlike that of typhoid fever, but it is distinguished from the latter by the appearance of its culture upon gelatine and potato, and especially by being extremely virulent to some of the experimental animals.

Again, from a clinical point of view, Psittacosis is liable to be considered as a typhoid septicæmia. But the point in question truly is a clearly differentiated bacterial disease which appears in the psittacea and which may be transmitted to man with the greatest facility.

Psittacosis is certainly more frequent than we have hitherto believed; it is often confounded with other infectious diseases, and MM. Gilbert and Fournier think that a number of outbreaks of epidemic pneumonia affecting a family or all the inmates of a house may be ascribed to Psittacosis.

Also it seems that a few prophylatic measures are necessary, such as inspection of fresh importations of parroquets, and instructions to bird-dealers and owners respecting these birds when they seem ill.—(*Translated from the Revue Scientifique, No. 18, 31st October, 1896.*)

INDIAN CIVIL VETERINARY DEPARTMENT.

Report of the Imperial Bacteriologist.

Dr. Lingard's report, dated May, 1896, is not so interesting as the previous report in which he settled the pathology of Surra upon a sure foundation. This is due to the fact that no man can constantly achieve success in research work within the period of a year, and to the circumstance that many months were occupied in removing and refixing the instruments and fittings of the laboratory to a distant and none-too-accessible town. The very thorough methods of the bacteriologist are mildly suggested by a statement that the goods moved by road and rail were of "the weight of 30 tons."

Dr. Lingard suggests that from the numerous drawings and micro-photographs of pathological subjects prepared in the laboratory, lantern slides might be prepared and sets distributed to each veterinary college in India and Burmah. This is a good idea, and, when adopted by the Indian Government, we hope every veterinary college in Britain will also be presented with a set. It would surely pay to make all the M.R.C.V.S. familiar with the special diseases of India before they arrive there to help the State in the suppression of animal plagues.

Some account is given of an interchange of opinions and specimens between Surgeon-Captain Bruce, of South Africa, and Dr. Lingard. The object was to determine whether any analogy or similarity existed between the Indian disease—Surra, and the African disease—Nagana, which is alleged to result from the bite of the Tsetse fly.

It cannot yet be said that anything is determined in this connection. After examining specimens from South Africa, Dr. Lingard cautiously says "That the flagellated protozoa found in the Nagana and Tsetse blood disease, corresponded exactly (so far as I am able to judge, microscopically) with those found in the blood of animals the subjects of Surra in India."

Referring to a copy of "*Preliminary Report on the Tsetse Fly Disease or Nagana in Zululand*," by Surgeon-Major Bruce, Dr. Lingard writes—"From the above report it appears that the fly disease is invariably fatal in the horse, donkey and dog, but a small percentage of cattle recover." Contrary to the experience of observers in India, the African disease is "characterised by the *constant* occurrence in the blood of an infusorial parasite, either identical with or closely resembling the *Trypanosoma Evansi*." The marked feature in the Indian disease being the presence of *paroxysms* and *intermissions* during which periods the trypanosoma is present and absent respectively."

Like all other reports concerning the health and disease of animals in India, this goes to show the fearful losses which are entailed by animal plagues. The camel suffers from surra. In reference to some villages which used to be noted for their camels and which used to be inhabited by camelmen, we read—"These villages used to be full of camels, but few remain at present and many of the people have been ruined from the losses caused by this disease."

The report concludes with some "Further notes connected with the Etiology of Surra" from which we take the following:—

Further Notes connected with the Etiology of Surra.

The course of this disease (Surra) is characterized by two kinds of periods: (i) *Paroxysms*, during which the hæmatozoon (Trypanosoma Evansi) is present in varying numbers, from 1 to 350,000 in each cubic millimetre of blood: (ii) *Intermissions*, during which the hæmatozoon is absent.

It may be stated, as a rule, that in all animals, the greater the number of hæmatozoa present in the circulation during a paroxysm, the shorter will be the duration of such period. The duration of a paroxysmal period in India varies between three and ten days, according to the animal attacked. In experimental animals (horses), when the disease has been transmitted by means of a trace of soiled blood placed in a slight superficial scratch, the paroxysms may last as long as 22 days, but the organisms rarely exceed 400 in each cubic millimetre of blood.

Although during the *intermissions* of Surra, the most careful microscopical examination of the blood has failed to discover the presence of a single hæmatozoon, nevertheless during such a period, lasting five days for instance, if a fresh guinea pig be inoculated with a like quantity of blood (0·3 c.c.) on each of the days making up the period, it generally happens that four out of the five animals will contract Surra. The blood extracted on the first or second day of the intermission, is the sample which fails to reproduce the disease in the guinea pigs. The period of incubation after such inoculation, however, varies considerably in length in animals of the same species. In one experiment when the blood of the second day of the intermission failed to re-

produce the disease, the latent period in the third guinea pig was 19·5 days, in the fourth guinea pig 8·5 days, and in the fifth animal 6·5 days, whereas in ordinary inoculations, guinea pigs injected subcutaneously with soiled Surra blood from the horse, contract the disease in from four to five days.

Successful inoculations have been made with blood taken from horses, which succumbed to Surra, up to 18 hours *post mortem*, but not later.

It has been previously noted, that the changes brought about in the blood by the hæmatozoon in Surra cases are—(i) high temperature, maximum 108°F.; (ii) rapid reduction in the number of red corpuscles in the blood; (iii) an increase in the number of white corpuscles; (iv) decrease in the number of coarsely granular oxyphile cells; (v) a diminution in the amount of hæmoglobin concurrently takes place, most marked during the paroxysmal periods.

When a Surra animal (horse) is treated with arsenic, the hæmatozoon disappears from the blood after a varying amount of the drug has been administered. The body temperature falls and reaches normal limits, and the animal frequently maintains its condition, sometimes putting on flesh. The red corpuscles increase in number, and again form normal rouleaux, a diminution in the number of white corpuscles, and an increase in the number of coarsely granular corpuscles takes place.

Inoculation has been successfully performed with blood drawn from an animal previously submitted to arsenic for a few days, from whose blood the hæmatozoon was absent; but after the ninth day of treatment, the immature form of the organism, if such it be, appears to be acted upon by the drug, for negative results then follow inoculation with the blood of the animal under treatment. There is, however, a tendency for the Surra hæmatozoon to re-appear in the blood of the animal subjected to arsenic, after an absence of about 25, 50, or 75 days.

THE SPREAD OF THE BUBONIC PLAGUE.

The following interesting abstract is taken from a report of a lecture delivered by Dr. Jas. Cantlie, before the Epidemiological Society, on December 18th last. The plague is now very prevalent in Bombay.

Infection of, and by, animals.—In support of plague being a soil-bred—a miasmatic—disease is the fact, so often announced, that rats are invariably affected. From Canton, Hong-Kong, Macao, Pakhoi, Yunnan, from hills in the north-west provinces of India, and, most recently of all, from Bombay the observation is constant, definite, and most pointed. The first time particular notice is taken of the death of rats in connexion with plague is in Mr. Rocher's report on the Yunnan plague, some twenty years ago.

Rats.—On all hands rats are reputed to behave peculiarly and with a wonderful constancy. Before, or it may be during, an epidemic of plague, or before the individuals in any particular house in an infected locality are stricken, the rats leave their haunts and seek the interior of the house. They seem careless of the presence of man and run about in a dazed way with a peculiar limping jerk or spasm of their hind legs. They are frequently found on the bedroom floor, or on the tables, but not unfrequently their death is known by the putrefactive odour of their decomposition arising from beneath the flooring. So pertinent has this rat affection become, that during the epidemic in Macao in 1895, the Chinese, and Portuguese left their houses when the diseased rats invaded their premises. The cause of the rats' behaviour is undoubtedly disease, and the symptoms tally wonderfully with plague symptoms of man. Dr. Rennie examined them carefully in Canton and noted the following post-mortem appearances: (1) the stomach was distended and filled with particles of food, sand, and indigestible substances, and the mucous membrane was red and inflamed towards the pyloric end; (2) the liver was much enlarged and congested and contained ova of tænia and distoma; (3) there was congestion at the base of the lung present in about 40 per cent.; and (4) glandular enlargement was present in 30 per cent. of those examined. There is no doubt now that the disease in the rat and man are identical. The bacillus of plague has been met with in every case of rat disease of this description where it has been searched for. The infection of the rat is raised from being a mere popular belief into one of scientific precision, and we must now accept the rat at any rate as one animal liable to plague. Whether the rat is affected previously to, coincidently with, or subsequently to, man being attacked is open to question. Is the disease amongst rats a forerunner of its outbreak in man, and, if so, are they a means of infection? These are, of course, two separate questions.

The rat seems to be infected before the human being; and the fact of dead rats being found about the house during the plague epidemic is a true warning that the inmates of the house will in all probability be affected. How is the rat infected? 1. Experiment proves *(a)* that the rat is inoculable and dies from a disease identical with bubonic plague in man; the inoculation of both the blood and inflammatory fluids of the plague-infected is effective; and further, a pure cultivation injected hypodermically also sets up signs and symptoms of rat plague and causes death within a few days; and *(b)* that supplying the animal with plague-infected food, whether flesh or cultivated germs, causes plague in rats. How does the rat in his natural state become infected; by inoculation? We cannot assume that every rat has a scratch or open wound by which it is infected. It is more likely he inhales or imbibes the poison either as a miasm in concentrated form or he ingests it with his food. It may be both forms of infection are at work; and, further, it must not be forgotten that rats are cannibal in their habits, and direct contagion must play a large part, if not the major part, in the general infection of their tribe. That it is some general infection and not a chance inoculation is manifested by their sudden and simultaneous seizure—a seizure so sudden that scores of dead and diseased rats will be found in a single house of any size within a few hours. An interesting observation in connection with the Calcutta affection is "that although Calcutta was believed to be the seat of plague and the plague bacillus was pronounced to have been proved in six cases no rats died." This was regarded as a peculiar and, shall I say, an unprecedented condition, but the explanation is simple enough. The rats did not die, as true plague did not exist in Calcutta, the cases of fever with bubonic swellings being found to be minus a toxic bacillus. This is in accordance with all previous experience. The Calcutta form of disease is benign and no record of rats dying during its prevalence has ever been made. Are we to doubt our belief in the diagnostic value of the bacillus or admit a new disease into our category—a benign polyadenitis? It may be we are called upon to do neither, for we have evidence to show that non-virulent forms of the bacillus can be cultivated and that a bacillus can be found in the soil and cultivated which behaves like the non-virulent bacillus extracted from true plague cultures. These points will be discussed later. 2. An observation by Dr. Thomson in the North-West Provinces of India is worth recording. "In these hill tracts the huts are usually double storeyed, the upper storey being occupied by human beings, and the lower by cattle, goats, etc. It is the custom of the villagers to store the grain in pits under the floors of the huts, and the soil is very foul from the excrement of animals &c. The theory among the people is that the

rats eat the grain which is often musty. Most of these hill people are very poor and would consume such grain, which is their principal food, even when musty and clearly affected with fungoid disease." It is some widespread source of infection by which the rat is infected. Dr. Rennie tells us that the places from which the exodus takes place are left without a rat to be seen.

Snakes.—Dr. Thomson remarks: "Moreover when rats are thus dying dead snakes also are said to be found about the site in unusual numbers." Later he remarks: "The snakes eat the rats." From the North-Western Provinces of India we also have record of snakes being affected during plague outbreaks. The snake is not observed as one of the animals affected in the list given by the Chinese, but the infection is natural, and the infection through their consumption of the rat is feasible. The possibility, however, of direct miasmatic infection must not be lost sight of.

Dogs.—In *The Chinese Customs Medical Gazette*, in the forty-seventh and forty-eighth issues we read that dogs died during an epidemic of plague. The observer ascribes it to the dogs eating the flesh of human beings or any other animal dead from plague. In the same paper it is described that in Yunnan the dying are removed out of the houses to repose in the streets in accordance with Chinese customs, and that those dead and dying from plague are found outside the dwelling or even outside the town walls. He has, moreover, seen the dogs eating the flesh of those dead from plague. In Hong Kong we had no special mortality amongst dogs, nor have Canton or Macao anything to say on the subject, but the dead were better cared for than in Yunnan and no dog could get at them. It would seem as though the dog were infected by eating the flesh. In the places mentioned such an opportunity was not afforded him, but in the town of Mentz, Yunnan, the custom as to the disposal of the dying seems to explain the infection of the dog.

Pigs.—It is even more important to ascertain whether or no the pig is infected. Pork is largely consumed by Chinese; and, moreover, Europeans in the East seldom or never eat Chinese-fed pork. The Chinese died from plague, whilst the Europeans enjoyed an immunity from the disease; nothing seems more conclusive than that pork was the infecting food. Moreover, most of the pork brought to the Hong Kong market comes from Pakhoi, a town where plague has been epidemic for years. The first point to be settled is—Does the pig suffer? Not as a rule. The pigs from Pakhoi showed no falling off in numbers, and they were not sickly; the former condition would have obtained had plague thinned their numbers in Pakhoi, and the latter would have been found out when they reached Hong Kong, where careful inspection of all food supplies is made. Dr. Mouchaud writes that the pigs died in Yunnan, and he states that "we saw on some roads dogs and pigs feeding undisturbed on corpses which no one cared to bury. These animals fell victims to their greed and succumbed to the scourge." Further, plague has been produced in the pig experimentally, all of which goes to prove that pigs die from plague after consumption of corpses dead from plague and inoculation; but that pigs are not primarily liable to plague in the same way as rats and pigeons seems conclusively shown.

Oxen, sheep and goats are not known to be subject to a disease allied to plague, nor are they affected by any fatal malady during the time plague is epidemic. As regards sheep, Custaldi states that plague in Kurdistan was preceded by a fatal epizootic among sheep; that they had plague, however, is not stated.

Jackals are known to be subject to the disease in Yunnan. They are more prone than either dogs or pigs to consume exposed human bodies, and therefore are very likely animals to be affected.

To sum up our knowledge, we find that pigs, dogs, rats snakes, jackals, and pigeons are affected by a fatal malady, whilst plague is epidemic amongst human beings. Pigs and dogs are believed to be attacked only after consumption of plague flesh. Snakes by the swallowing of rats, and possibly jackals are infected in the same way. None of the purely herbivorous animals, horses, donkeys, oxen, sheep, goats, or rabbits, are attacked. It is the flesh-eating animals which are the sufferers. Of course the rabbit, guinea-pig, and a number of animals can be inoculated experimentally or made to consume plague-diseased food, but no herbivorous animal acquires it by the natural processes of infection. If it is by consumption of flesh that animals are infected, how do human beings get it? The animals affected seem to become infected from flesh: the snake from the rat, the jackal, pig, and dog from eating the flesh of persons dead from plague; but how does the rat become infected? Could we find that out we should likely know how man is infected. One animal missing in the scale is the cat; if rats perish from plague surely the mortal enemy, the cat, would be diseased. Perhaps the instinct of the cat teaches it to avoid the diseased rat. [In Hong Kong during 1894 many cats died in a peculiar way—the chief symptom being paralysis of the hind legs].

In the discussion about the rat infection it must never be forgotton that if we take it for granted that the disease attacks rats before human beings become victims, we must consider the question, "Has the rat ailment to do directly with the subsequent outbreak in man?" It is very difficult to see how the rat could affect man except by *infection*; and one may hesitate to believe, from what we know of the concentration of poison necessary to infect, that such is possible. It might be that the diseased rat, by *treading*, or dropping upon, or biting the food of man, would thereby poison the food. It will at once occur to one that sick rats, like sick men, would not eat. This we have no proof of; in fact, it is the other way, for Dr. Rennie states that in his post-mortem examinations on rats in Canton he found the stomachs pretty full of food, and rats during incubation and first days of illness from experiment eat fairly well. What we may conclude from the study of the infection of animals is: (1) that the rat is the animal most liable to be attacked by plague; (2) that rats suffering from, or dead from plague may infect other animals, such as snakes and jackals, who consume them; (3) that rats are always affected by a disease similar to plague at the same time man suffers; (4) that the rat may infect man, but the means of conveying the contagium is not known; and (5) that during the prevalence of benign plague, pestis minor, rats do not die.

Incubation.—The time intervening between exposure and the development of plague has been carefully observed and noted throughout many epidemics of this century. The conclusions arrived at by all who have written an account of their Hong Kong experience gives from three to six days as the period of incubation. Dr. Lowson makes it seldom under three or over six. Professor Aoyama gives it at from two to seven days. If by that statement he means from the end of the second until the beginning of the seventh day his recorded observations agree exactly with Dr. Lowson's and others. Only one case seems to break the record: it is a case referred to by both Dr. Lowson and myself. A Chinaman in the gaol at Hong Kong developed plague after being nine days under lock and key. From June 11th until the 20th he was apparently well, but on the evening of the 20th he became feverish and developed plague. This case gives nine days as the apparent period of incubation and may be taken as the limit in that direction. Dr. Lowson states, however, that the man complained of being ill for two days before being placed on the sick list, thus reducing the period to the seventh day; and although the man was put in the cells on the 11th it was

not until the 12th that he was really beyond contact with the outer world.

The occurrence of several cases of pestis minor in Calcutta, in which the bacillus was found, indicates a more cautious acceptance of diagnosis resting solely on bacillary evidence than we have heretofore contemplated. The first case dealt with and reported upon in Calcutta gives an instructive history. The patient, a lad aged seventeen years, came from Bombay, where evidently he had been exposed to infection, as his sister, who accompanied him, had seen several cases of plague in Bombay. Fifteen days before leaving Bombay he had noticed swellings first in one groin and then in another, but never felt ill until his arrival in Calcutta on Sept. 24th. He was seen and carefully examined in Calcutta by honest observers, and a diplobacterium identical with the Kitasato bacillus was found in his blood. Not only so, but the clinical symptoms of plague were most manifest. It would, therefore, seem from this well-authenticated case that glandular swellings may attack a dweller in a district where true plague rages, and that at a subsequent period may become virulent and kill the patient by a true plague; in fact, that a pestis ambulans may become a malignant polyadenitis in the same person, even although the patient is removed from the infected zone This may be true, although it is not at all a comfortable doctrine; but I can come to no other conclusion having regard to the evidence before me. The incubation period of plague, therefore, may be fixed as from three to six days whilst an epidemic rages, but the evidence to hand, within the last few weeks from Calcutta renders the period of incubation of ambulatory plague uncertain.

Invasion.—The onset of the disease in malignant polyadenitis is known to be usually sudden and severe. The patient is seized with headache, fever, and prostration and compelled straightway to seek his bed. In a few hours or a few days a swollen gland developes and the disease runs its course. But we have on record many cases where high fever existed and the patient did not even feel ill, or where a gland swells up with no general feverish state. Dr. Lowson in his valuable report states: "In one case I took the temperature of an Indian who *looked* ill, but who had merely come to see about the burial of a friend and who complained of no unpleasant symptom. He was rather amused at my using the thermometer, but I found he had a temperature of 103° F. and at the same time discovered a small cervical bubo." Be it observed, as bearing upon the intensity of the virus, that this was a non-fatal attack of plague. In many cases a bubo may precede the fever, the patient suffering no inconvenience even. Such a case is that recorded from Calcutta, where "the patient, a boy from Bombay, had had for fifteen or twenty days before developing fever a swelling in either groin. Subsequently he developed the ordinary symptoms of plague and the diplococcus was found in his blood." These two cases are sufficient to show that fever or plague may exist and the patient not be aware of it, and that glands may be affected for a fortnight before the disease declares itself in the usual way by headache, fever, and prostration. The onset of the disease, therefore, even during a plague epidemic, may not be so definite as we should like to believe, and as yet we have no charts recording the condition of the patient between the time of exposure and the invasion.

Varieties.—It is not my intention to mention merely the varieties of symptoms which predominate at any one time or throughout any one epidemic, but to draw attention to varieties which are so pronounced in their character as to justify the belief that whilst dealing with this subject we are actually attempting to bring in line under one heading diseases which may be even separate species. The commonly accepted types are these: (1) abortive (or larval) plague; (2) typical plague: and (3) fulminant plague. The two latter are so well understood and have been so largely and fully dealt with elsewhere that there is no advantage in recapitulating what can be read in any text-book; but I wish to draw attention to the first named. The abortive form is sometimes styled larval, ambulatory, or pestis minor. The names partly declare the character of the disease and create a condition so apart that it has at all times been separately described. It is at the same time so indefinite in its duration, so unstable in its relation to malignant plague, and so uncertain in its onset and departure that up to the present no scientific precision has been assigned to the relationships. As long ago as the year 1666, when plague ravaged London, we have it on the authority of Sydenham that "at times tumours may break out without either fever or any important symptoms. . . . Those to whom this happens may walk about in public as usual and attend to common duties of life. . . ." Foderè, writing of plague in 1820, observes: "In the Levant and in the Marseilles epidemics of 1820 cases were to be seen which were not ushered in by any alarming symptoms and where the natural functions were undisturbed and where buboes and carbuncles appeared without fever or only with slight fever, or the buboes went on to a healthy suppuration more or less prompt, or even disappeared and went on to resolution without the help of art, without any inconvenience, and with a perfect integrity of all the functions. This state is comparable to benign small-pox, during which children play together and walk in the streets without any precautions, no care being taken of their treatment and yet terminating favourably. It is the benign plague of authors which is observed when the disease commences and when it is at its end, though it is rarely seen in the middle period, which is entirely devastating, *but it is not less plague and it no less merits the attention of physicians and magistrates.*"

The bacillus.—The bacteriology of plague began with the advent of Kitasato from Tokio during the Hong Kong epidemic of 1894. On June 14th, 1894, Kitasato demonstrated the bacillus, which was confirmed afterwards by Dr. Yersin of the Pasteur Institute in Saigon, working in an independent manner. The bacillus has been variously described as (1) a diplococcus; (2) diplococcus enclosed in a delicate capsule; and (3) a short bacillus with the ends somewhat rounded and a clear space or band in the centre. Professor Woodhead writes: "The size varies considerably according to the staining method employed, the organism appearing rather smaller in the watery gentian violet solution than in the glycerine fuchsin solution. It is plain that the size of the capsule and its staining capacity vary very considerably, and it is quite possible that this appearance of a capsule is like that which is sometimes met with around the bacillus of anthrax stained in special solutions in that it may be simply a refraction from the albumin from the constricted bacillus due to the drying and other processes of preparation." In the figures drawn by Woodhead an organism was met with in some specimens which seems to indicate a development after death. The last statement may be the most important of all, and its confirmation will be anxiously looked for during the present outbreak in India. It may be the first step towards a knowledge of the life history of the bacillus in the same manner as the flagella of the plasmodium malaria are found to develop in the field of the microscope some half hour after removal from the body. Dr. Yersin undertook some experiments to test the truth of the infection of the soil, the results of which elucidate the toxic nature of the bacillus. He found (1) that a bacillus in some respects resembling in its appearance that met with in the human body was found in the soil of Hong Kong during the plague epidemic; (2) that the bacillis of the soil and body occasionally behaved similarly in culture experiments; and (3) that the culture specimens

of soil bacilli were, however, innocuous, possessing no virulence when injected into mice or guinea-pigs. Dr. Yersin further found that in his cultures of human plague bacilli there were microbes of different virulence, some mildly powerful and some altogether powerless to infect either mice or guinea-pigs. Dr. Lowson, in conjunction with Dr. Takaki, Dr. Kitasato's assistant, found a bacillus in the soil of the garden near his house which closely resembled the human plague bacillus, possessing most of its microscopic attributes, but showing none of its toxic properties. The district whence the soil was obtained lay within the plague-stricken area, but considerably removed from any infected house.

The conclusions to be deduced from the above rehearsal are :—(1) That plague bacilli vary in size ; (2) that cultivation experiments produce bacilli identical in appearance but possessing different toxic and infective powers ; and (3) that a somewhat similar bacillus is found in the soil (whether at all times or not is not known) which resembles the non-virulent form the result of bacteriological culture.

Infection of the soil.—The preceding remarks upon finding the bacillus in the soil are pregnant with suggestion. Whether the soil bacillus be virulent or not, whether it be completely identical with the human bacillus met with in plague or not, the fact of a microbe having on the one hand a close resemblance microscopically and on the other hand a structure and behaviour analogous with certain of the non-virulent cultures products of human plague bacilli, is sufficiently important, and brings the elucidation of the matter within the sphere of practical investigation. The medium of the soil may be to the infection of animals as the medium of water bearing the carcase of the dead mosquito is to the filarial-infected human being. Not only may it serve as a mere vehicle, but as a host, in which the parasite passes through a stage of its evolution. Bearing closely upon this subject is the question of the *medium* of spread of plague. I previously discussed this shortly when dealing with contagion. Contagion—that is, intimate prolonged contact, as mother or nurse and child—no doubt is a factor, a potent factor, it may be the only factor, in the transmission of the disease, but few admit it to be more than a small element in distributing plague broadcast over a community. The slow progress of plague and its peculiar paths of selection must be considered if the subject is to be fairly dealt with. Plague is perhaps the slowest travelling disease known ; it may take months and even years to extend a few miles. From the City of London it took nine months to reach Soho, and more recently it took ten months to travel from Hong Kong to Macao—a distance of thirty miles. The lines of communication will not explain this, for in Macao thousands of persons sought shelter in 1894, when Canton and Hong Kong were plague stricken. Steamers and Chinese junks and fishing boats passed freely between the ports, yet Macao did not become infected until nine months after Canton and Hong Kong were pronounced clear. How did the parasite reach Macao—by sea from Hong Kong or by land from Canton ? If by sea, in all probability it would have been in Macao long before. If by land, how did it travel—by human or animal carriage or by the soil ? If by human beings, it means intimate prolonged contact of the carriers, if by the soil it means a bacillary infection of the soil, spreading from a centre and extending by a continuous margin of active growth along paths favourable to its nutrition. As a gangrene spreads from a centre, or a patch of ringworm starts to spread and gradually involves an ever-increasing zone, so may the soil become sown with the germ of plague, and animals that have their habitat thereon fall victims. Some show of testimony towards such a view is afforded by the fact that plague confines its working to the Old World and does not cross the ocean, and travels backwards and forwards in the belt of its selection ; but it cannot be the only means of travel, otherwise how did the islands of Britain and the Canaries ever become infected at one end of the Old World and the islands of Hong Kong and Formosa at the other ? Were the soil only the producer or infector all islands in the area ought to be immune. This not being so, it comes that the poison must be carried by human beings, by animals, by clothing, or by food. That the poison conveyed by them may require the soil as a medium to reproduce the infecting bacillus may be quite possible ; but that some other medium than the soil is capable of transmitting the disease is quite evident. As noticed above, the bacillus found in the soil, according to Dr. Yersin, agrees in most points with the plague bacillus except that when introduced artificially into the bodies of animals it is non-virulent. This may indicate, of course, that they belong to another group of the bacterial family altogether.

The conclusions drawn from a study of the spread of plague are :—

1. The varieties of plague known under the names (*a*) fulminant, (*b*) typical, and (*c*) pestis minor, are allied. 2. The cause of fulminant and typical plague is a diplobacterium in the blood and tissues. The cause of pestis minor may be an allied diplobacterium, but with a lesser toxic power. 3. An appropriate name for the fulminant and typical plague is "malignant polyadenitis." An appropriate name for the mild variety (pestis minor) is "benign polyadenitis."

1. Plague is infectious chiefly by the dust arising during the cleansing of dwelling houses which plague patients have occupied. 2. Plague is contagious by prolonged and intimate contact with the plague-stricken, as in the case of a nurse carrying a child ill of the disease.

1. Plague is met with in a definite area of Asia which may be termed the "plague belt." 2. That the home of plague at the present day is Mesopotamia and the countries adjacent. 3. From Mesopotamia as a focus the plague may spread northwards to the Caspian Sea, westwards to the Red Sea, southwards as far as Bombay, and eastwards as far as (Formosa) the China Sea. 4. During the present century plague has shown a western retrocession and an eastern accession of virulence.

1. Typical plague (malignant polyadenitis) is associated with pestis minor (benign polyadenitis). 2. A bacillus of somewhat similar appearance microscopically is reputed to be found in both. 3. The bacilli differ in their toxic powers only (?) 4. A benign polyadenitis may run its course without being preceded or followed by the malignant variety. 5. Malignant polyadenitis may run its course without being preceded or followed by the benign variety. 6. The bacillus of the benign variety attains malignancy by passing through some intermediate host, possibly, but not probably, the rat.—*The Lancet.*

VETERINARY SOCIETIES.

THE CENTRAL VETERINARY MEDICAL SOCIETY.

A meeting was held on Thursday night the 7th inst., at the Royal College of Veterinary Surgeons, Red Lion Square, Bloomsbury, under the chairmanship of Vet.-Lieut.-Col. J. Kettle, president of the Society. The other fellows present were : Messrs. W. F. Barrett, J. Baxter, A. Clapp, J. A. W. Dollar, Vet.-Col. G. Durrant, A.V.D. ; T. C. Garry, Vet.-Major Duff Gillespie, A.V.D ; Professor F. Hobday, W. Hunting, Vet.-Capt. A. J. Haslam, A.V.D. ; W. King, Professor Macqueen, J. H.

Manton, F. C. Mahon (hon. sec.), Hugh A. MacCormack, Vet.-Capt. Joshua A. Nunn. A.V.D.; Professor Penberthy, W. Perryman, Professor Pritchard, J. E. Rickards, A. Rogerson, J. Rogers, W. Roots, J. Rowe (hon treas.), and friend, H. Singleton, W. E. Taylor, J. E. Tooth, T. Upton, S. Villar, F. O. L. Walpole, F. Willett, F. W. Willett, F. W. Wragg; and as guests of Professor Hobday: Dr. Easterfield, Dr. Jowett, and Mr. Marshall.

On the motion of Mr. W. F. Barrett, seconded by Mr. J. Rowe, the minutes of the preceding meeting were taken as read and were confirmed.

The Hon. Secretary announced the receipt of communications from Mr. M. Clark and Mr. J. R. Cox expressing regret at their inability to be present.

Election of Fellows.

A ballot resulted in the unanimous election of the following gentlemen: Vet.-Major G. Durrant, A.V.D., Woolwich; Vet.-Major S. Duff-Gillespie, A.V.D. Woolwich; Vet.-Captain Joshua A. Nunn, F.R.C.V.S., C.IE., D.S.O., A.V.D., Woolwich; Vet.-Captain A. J. Haslam, B.A., M.D., F.R.C.V.S. A.V.D., Woolwich; Mr. F. R. Stevens, M.R.C.V.S. Redhill; Mr. H. King, M.R.C.V.S., 155 Kennington Cross, S.E.; Mr. C. H. Welfare, M.R.C.V.S., Tudor House, Staines; and Mr. J. Nettleton, M.R.C.V.S., Hammersmith Broadway.

Indian Hemp.

The discussion was resumed on the paper by Vet.-Capt. Rutherford, Umballa, read at the preceding meeting by Professor Macqueen.

Mr. W. Hunting: Gentlemen, it is my misfortune to have proposed the adjournment of the discussion on Captain Rutherford's paper, at our last meeting. It did not then occur to me that the member moving the adjournment was expected himself to re-open the debate on the next occasion. Had this fact been present to my mind at the last meeting I certainly should not have proposed the adjournment of the debate, because I know nothing whatever about "Indian hemp." I have never used it, and I have only seen it once or twice. However, my position enables me to say that I think we all agree in this: that any man who does a piece of work, such as Captain Rutherford has done, deserves the thanks of the whole veterinary profession. There are very few men who will take any drug and devote so much time to it, and make such careful experiments and records as he has done. At any rate his work has added something to the science of veterinary therapeutics, if such a science exists—for I am a little doubtful whether there is a science of therapeutics. If I had to give an opinion I should say there certainly was not such a science, and that therapeutics at the present time means a mass of empirical observations which, as yet, have failed to be properly digested. Still, the paper before us is a contribution towards more exact knowledge. As to the action of this Indian hemp, it seems to me, from Captain Rutherford's account of it, the drug is one as to which it is extremely difficult to make up one's mind. It would be difficult to say in what cases it is really useful, and in this connection you will note that Captain Rutherford's experience is not borne out altogether by the experience of others. We have been told by other authorities that Indian hemp produces no constipation, and is therefore a useful sedative in cases of intestinal disease. Now, in the horse, I take it, there is a special liability to intestinal disease being accompanied by constipation. In horses nearly all our cases of so-called "colic"—using the word in its widest sense—are accompanied by a tendency to constipation and very often by distinct impaction of the large intestines. Captain Rutherford's cases are in nearly every instance accompanied by—I will not say constipation—but by want of action of the bowels. In some of the experiments referred to by him, twenty hours elapse without a horse passing any excreta whatever. If that is the action of Indian hemp it seems to me that, in cases of colic, in all those cases in which impaction and constipation is likely to be present, Indian hemp is to be avoided; and yet we are told by some authorities that it is in these very cases of colic that Indian hemp is good. Even if Captain Rutherford's experiments have not demonstrated anything definitely—have not shewn that the drug has any specific action in any special disease—I think one thing runs through the whole of his experiments, namely, that Indian hemp does stop the action of the bowels. Hence in all cases in which that is detrimental, we should avoid the drug. That appears to me to be the chief teaching of Captain Rutherford's experiments.

The drug produces stupor in all of the cases instanced when the dose reached a large amount. In smaller doses, as soon as the horse was moved or attempted to be moved it became greatly excited, as did the horse which stood in the stable almost asleep, but which was nevertheless capable of being brought out and trotted. Our President told us something of the subjective symptoms of the drug in man, but I think the subjective symptoms in the horse are worth some consideration. They suggest more terror than pleasure. The terror of movement of anyone approaching or touching the animal seems to me a disadvantage in the use of the drug. Certainly the moral I draw from Captain Rutherford's experiments is, the less Indian hemp we use the better; and that up to the present time we have arrived at no such knowledge of the drug as will enable us to say it is of use in any case, or to deny that it may be injurious in every case. For all that I acknowledged that the work done by Captain Rutherford is valuable, as all experimental work when carefully recorded adds to the sum of our knowledge.

Mr. Upton much questioned whether anodynes were much called for in the majority of cases of colic. When, in 1893-4 he was in practice in the Midlands, hay being very dear and straw largely used as a substitute, cases of known impaction of the colon came under his notice very often. At first he tried anodynes, but found they gave very bad results. His predecessor tried nux vomica and found that that, combined with stimulants, yielded a satisfactory result. When they talked about colic they meant a spasm of the colon. He thought he was right in attributing colic, so-called, to the colon. If they took the bowel and noticed the way in which it turned, nobody need be surprised at cases of impaction of the colon. If one put his hand into the rectum in cases of impacted colon he would find a hard mass. Mr Upton went on to say that in cases of diarrhœa castor oil answered well in the human subject, and he had seen it stated that Epsom salts proved efficacious in cases of diarrhœa. Indian hemp might, however, be useful in cases of colic, but he did not think it was any use administering this drug in the form of a tincture as the horse would not swallow it. The only way was to give the drug in the form of a ball consisting of two or three drams of the extract.

Prof. Hobday said they were all very much indebted to Captain Rutherford for having taken so much pains in making and recording experiments with Indian hemp. Personally, he had read the Captain's paper with more than usual interest. During the last two or three years he (Prof. Hobday) had had opportunities for using this preparation and he had employed it very largely both in the horse and the dog. He had, however, failed to notice the excitement which Capt. Rutherford observed in horses, even when they had had very big doses. In fact the opposite had invariably been the case, both in the horse and dog. Whenever they had had a dose of Indian hemp, administered experimentally or medicinally, for a considerable length of time he had been unable

to see any of that excitement which was usually noticed in the human subject. Captain Rutherford mentioned that large doses of Indian hemp caused constipation in horses This he (Professor Hobday) regarded with considerable interest because he had always understood that the opposite was the result and that Indian hemp was of more value than morphia particularly because it did not cause this constipating effect. When Indian hemp and a dose of aloes were administered his belief was that the bowels were not moved as all until a considerable length of time afterwards, the hemp arresting the movement of the bowels. He had noticed different degrees of action resulting from the uses of Indian hemp. One pony for instance, receiving four drams of the drug, would become stupid more quickly than another pony having the same dose and being similarly conditioned. The like he had observed, too, in dogs. His experience was that a constipating effect did not occur in dogs when the drug was given in small doses. He had administered to dogs doses of Indian hemp varying from ½ up to 4 grains and had not noticed any loss of appetite nor constipation. With reference to the toxic properties of the drug he had noticed that animals behaved very differently when very large doses were given, but when, in the case of dog's he had tried with doses varying from 10 grains to 2 drams to poison them, he had not yet had a successful result. The worst thing that had happened to them was that they had been reduced to a state of stupor a couple of days with paralysis of the hind quarters. Eventually they had recovered and become quite well again. With regard to antidotes he mentioned that he had several times used cocaine as an antidote and he could in every way recommend it for waking an animal up. Therapeutically it could be reasoned out quite correctly as an antidote to narcotic poisoning from such a drug as *Cannabis Indica*. Its effects were rapid, and, although they were somewhat transient, the patients in which he had used this antidote had been frequently benefitted by its administration. They had been roused out of their state of stupor, and the benefit had been permanent. About eighteen months ago he had a conversation with Mr. Wood, of Cambridge, a gentleman working in a laboratory there, and he suggested to him that he could not do better than turn his attention to *Cannabis Indica* with a view to preparing it in such a form that it could be used hypodermically in the treatment of colic. *Cannabis Indica*, as they were aware, was dirty stuff to make up, and if they got it on their hands it was difficult to remove it for some little time. Well, Dr. Easterfield, Dr. Jowett, and Mr. Marshall, of Cambridge, (gentlemen who favoured them with their presence at that meeting) had, in conjunction with Mr. Wood, done good work in mak ing different preparations of the drug. The results obtained with one preparation in the human subject, had not, however, been realised in the lower animals. The results of experiments in horses and dogs were so far unsatisfactory. Dr. Easterfield in experimentalising on himself, had, however, obtained satisfactory results.

Dr. EASTERFIELD said it was with some diffidence that he appeared before them that evening, for he could not claim to hav any knowledge of veterinary science. Professor Hobday asked him to attend the meeting, and he also received an invitation from the secretary of the Society. He thought it might be of interest to them if he described how far their experiments had at present gone in the way of the isolation of pure chemical substances from *Cannabis Indica* which might be used either in veterinary or other medicine. He had brought with him samples of various hemp products because he thought it was just possible some of them might not have seen the crude form of the drug. In his paper, Captain Rutherford alluded to *Cannabin* a substance prepared by those chemists to whom veterinary science owed so much—Messrs. T. and H. Smith, of Edinburgh. From the preparation of Gunjah—the dry product produced in India. They prepared a resin called cannabin. They found that cannabin produced all the usual symptoms of hashis poisoning, that was to say, by giving a dose of something like 5 grains they were able to induce toxic effects and intoxication, and a larger dose produced catelepsy, that was, of course, in the human subject. He (Dr. Easterfield) and his colleagues, did not begin by experimenting on this. They consulted gentlemen in India who informed them that charas was the substance which the natives of India considered the most toxic form of the drug, and, therefore, they procured from India 15 or 20 lbs. of that material. Now this charas (of which he exhibited two samples) was a nasty looking substance. It was prepared from the fronds of the female hemp. A native dressed in hides, walked through the crop and the resin stuck to to him. It was about the most obstinate stuff to remove from the body that he had had to deal with. In order to reduce the substance to a form in which it could be handled and sold and made marketable, it was mixed with a certain weight of earth or sand; and to increase the bulk it was only necessary to have a large admixture of sand. They found that some samples contained as much as 70 per cent. of sand. The resin abstracted by means of alcohol was usually solvent. From this resin they had been thus far able to isolate from different chemical compounds. The process they used was practically one of dissolution. In the first place they obtained the rather sweet smelling substance—terpenes. In a hotter temperature "charas" itself was recognisable. Then they obtained a higher boiling point. Finally they got a distillation at 400° (c) of cannabinol, which, as he had experienced, was productive of the symptoms of hashish intoxication. He took a dose of three-quarters of a grain in the form of a pill, and in the course of an hour found his saliva beginning to thicken. Without going to sleep his brain became a kind of kaleidoscope, but if he tried his brain could not recall any part of the vision that had passed. It was a delightful kind of intoxication similar to that induced in Indian hemp users in India. Another substance they isolated was paraffin, which had no physiological action. The conclusion at which they arrived was that, given the isolation of these different substances out of simple charas, there was no doubt the hemp plant contained other active substances, just as in the case of the poppy plant not only morphia but other substances as well were present. The substances they isolated, it was hoped, might be useful in cases of colic in animals, and other similar ailments. But as a matter of fact, as Professor Hobday mentioned, the required effects were not forthcomming. In their further experiments they hoped to obtain other substances which might have the desired effect in colic, &c.

Mr. MARSHALL stated that, after making a few investigations upon rabbits and cats with the substance isolated by Dr. Easterfield and Mr. Wood, without finding it very toxic, he took a small dose of the stuff himself. He described his resultant sensations an hour after, how he first felt a peculiar sensation in the mouth, thickened and dry saliva, a disposition to laugh for no apparent reason, and how finally he became beastly drunk. He could not speak properly or walk straight. He had no particular illusions but a most peculiar effect was the loss of time relation. He could not tell what had passed a minute before. Ten seconds appeared to be an hour. The intoxication lasted two or three hours. Afterwards he seemed to be recovering. A marked effect of the drug was the lassitude it induced. The intoxication was not even throughout, at one time he was laughing and rushing about the place and the next moment he was almost rational. In fact he was able to discuss things in general and was even looking after the laboratory work to a considerable extent. After two or three hours he

took a little coffee, went out and by the time he returned he had recovered. The effects were the result of taking about 1½ grains. Afterwards he took another and a smaller dose without any resultant action for about four hours. He got again the same condition of saliva, and this time had both illusions and a tendency to sleep. He subsequently took 1-6th or 1-7th of a grain but the stuff had practically no effect whatever. Adverting to his experiments on animals, he said he started with rabbits. He gave 1 dram—a fairly large dose—and found beyond a very slight excitement, the stuff had no action whatever. With 2 drams of *cannabis Indica*, which Dr. Easterfield sent him he got a slight effect. There was a depression of temperature, a fall of the respiration rate and the animal had a tendency to sleep, but this very quickly passed off. He gave a cat about two grains and it had a very marked effect. The animal suffered from sleep, from ataxia, weakness in the hind legs, and subsequently thick salivation. The dose was extremely small and the effect lasted about a day and a half. After the cat had practically recovered four or five days he repeated the observation, this time giving the animal 3·7 grains. Sleep ensued, followed by extreme weakness and ataxia. The animal hardly lifted its head and its tail was spread out. This lasted for a couple of days. Of course the cat could get up and move about. The strong effect was really the salivation which came on after about a day. The saliva ran down in streams from the cat's mouth, and this continued to the end of the experiment. Afterwards he gave the cat a very much larger dose and just the same effects were produced save that they lasted longer and were somewhat more marked. The animal ultimately recovered and he was unable to kill it with even a larger dose. The cat had rather a less stool than usual. So far as he was concerned he suffered very little diarrhæa from use of the drug after a small dose of it. He believed that after the large doses the diarrhæa—the laxative action—in the act was not so marked as in the small doses. Here he was speaking only from memory. A number of interesting points pharmaceutically arise out of the use of *cannabis Indica* upon the animal organism but these he should not then deal with. They were practically absolute pharmaceutical questions and might not be of interest to that society. Of course from his own experiments *cannabis Indica* should prove a valuable agent in colic in horses. He hardly understood what colic in horses meant. It seemed rather peculiar to him. In colic generally with constipation he thought that *cannabis* would have a very useful effect.

He then pointed out that cannabis was apt to deteriorate by keeping, and some kept in a cupboard two years exposed to the air was found to have practically no effect. It was suggested that cannabis became oxidised or changed in some way, which was extremely probable. Before they could be absolutely certain that cannabinol or any cannabis preparations would prove of service therapeutically, they would have to know a little more of chemistry. And he thought they would do that before very long. They must have a definite compound in the market, and it remained to be seen whether cannabinol would, in course of time, lose its physiological properties. With regard to its therapeutical use, he knew that it had been used very largely in medicine; and, with respect to the constipation of horses, he knew that it had been used not very long ago by some doctors practising in India, for dysentry and various intestinal conditions of that kind. It might be that whereas these substances had a definite effect in producing constipation, cannabinol oil used in proper doses had no such effect.

Mr. Barrett said he thought they would all agree with him in welcoming Dr. Easterfield and Mr. Marshall there that night. They had been much interested in their statements and in seeing the chemical products they had obtained from Indian hemp. Personally he knew nothing of the action of *cannabis Indica*, and attended that meeting for the purpose of gaining information of the drug. That the drug was a peculiar one was, he thought, plainly manifest inasmuch as in some cases it had a marked action, while in others it had practically none. It was not often that medical gentlemen operated upon themselves but Dr. Easterfield and Mr. Marshall were brave exceptions. In regard to the paper of Vet.-Capt. Rutherford he thought that gentleman was entitled to their thanks for the very considerable care he had exercised in its preparation, and for the pains he had taken in discovering the effects of this curious drug. Before they, as veterinary surgeons, made up their minds as to the use of it, they should ask themselves this question: Did this drug possess any special advantage over other drugs which were at their command? Had *cannabis Indica* any action beneficial to their patients which was not observable when they used morphia or chloral hydrate? From the experiments made and related it appeared to him that this was not the case. His belief was that in morphia and, perhaps, chloral hydrate and other drugs, they had agents which were superior to *cannabis Indica*. Hence, speaking as a member of the veterinary profession, he thought they should hesitate before they discarded agents in which they had every confidence, in favour of a drug of which they knew so very little. The only point in which, so far as he could see, in *cannabis Indica* was more beneficial than morphia, was in regard to the duration of its action. Captain Rutherford pointed out that the effects of *cannabis Indica* lasted seven hours. They had the slow action between the administration of the drug and the apparent effect upon the patient, and that was a reason why in the early stages of colic or painful intestinal complaints this drug should not be used. They might have cases where the pain was more acute and where to leave the animals affected by a sedative for two, three, or four hours would probably result in death from exhaustion. With respect to the action of this drug upon the pulse, it seemed nothing definite could be relied upon. In some of the cases frequency of pulsation was accelerated, while in others this effect was not produced. From what they knew of the nature and effect of *cannabis Indica* it seemed it could not be safely resorted to in those painful diseases of the horse in which the use of morphia was warranted. He was surprised that some gentlemen denied the constipating effects of *cannabis Indica*. It appeared to him (Mr. Barrett) that in every experiment to which Captain Rutherford had recourse, constipation ensued. Veterinarians observed in their patients that this condition resulted from the administration of any powerful sedative. Speaking broadly he thought there was no doubt that any agent which was capable of sleeping the brain, in its turn indirectly slept the bowel, and in a sense the action of any organ of the body. They found, generally speaking, that not only *cannabis Indica* but morphia, chloral hydrate and other sedatives had a marked arresting power upon the bowels. The bowels acted less frequently, the discharge was less copious, and was apt to become coated with discharges from the mucous membrane. Mr. Hunting, in his remarks, had rather ridiculed the science of therapeutics. He did not agree with him. At the same time he admitted that the science of therapeutics was unsatisfactory inasmuch as they seldom got the results from drugs which they anticipated. As one grew older he found that medicine was less valuable than he imagined when leaving college and college training. Nevertheless the time had not yet arrived when they could afford to discard medicine. They knew that there were affections of the horse and other animals in which medicine gave very marked and speedy relief. While medicine was able to do that they could not afford to discard altogether the science of therapeutics. Mr. Upton, in his remarks, rather diverged from the point. He advocated not the

use of anodynes but of stimulants. But they were there discussing the action of *cannibis Indica* and seeking to know whether they would be justified in using that drug. He listened to the observations of Professor Hobday very attentively, as he always did to any speech of that gentleman. He appeared to have interested himself in certain experiments but, curiously enough, the results he obtained seemed to be just the opposite of those reached by Captain Rutherford. And this tended to shake one's confidence in the drug. An agent which, when administered, sometimes produced sleep, occasionally excitement, sometimes diarrhœa, and sometimes constipation, did not create much confidence in its action. It was these uncertain results which impelled him to say, as a veterinary surgeon, that they had yet much to learn about this drug before resorting to it to the exclusion of morphia which they had well tried and well approved of.

Prof. PRITCHARD said that on a previous occasion when he was invited to speak upon this matter he declined to do so because he really knew little or nothing about it. That night he was in the same position, but some remarks which had been made in the course of that discussion induced him to rise. First he would remind some of the gentlemen present that this subject had not been under the consideration of the veterinary profession for merely a week, or a fortnight, or a year. As early as 1859 he heard the late Professor Morton lecture upon this very subject. That brought him to think that if there was anything extremely valuable in Indian hemp something more definite than had yet been developed would have been brought to the surface. He had listened very carefully to Captain Rutherford's paper, so admirably read by his friend Professor Macqueen, and when he had listened very carefully he thought they would credit him in having done something. After listening to the paper he thought there was something very questionable as to the way in which this material was collected. Was the material collected in a low or a high ground, at one time of the year or at another? Was it produced in the form of a liquid or a solid? Then with respect to the doses, he could not but come to the conclusion that there was extreme variation in the amount given by one individual and that advised by another. Then in reference to the symptoms produced by the effects of the drug nothing could be more strange. It had been said that evening in one instance that it produeed purgation and in another instance that it produced the very reverse. In some instances they had had an elevation of the pulse while in others the very reverse was the effect. Putting that on one side, was the drug to be depended upon from another point of view? They had heard nothing definite about the dose to be given, whether it was one proportion or another; and as had been already said, were there not drugs upon which they could rely that would produce their effects in a very short time? Take chloral hydrate for example. Scores of times had he administered doses of this drug and seen the effects occur within half-an-hour to an hour. In one instance he had put a donkey under the influence of chloral hydrate and had operated upon it for half-an-hour, after which the animal perfectly recovered. Now, if they could do this in a case of colic without driving their patients into a state of *delirium tremens*—for that had been the condition exactly described that evening—he thought they had drugs at their command that were far better—at all events equally as good as *cannibis Indica*. As he said at the commencement. he knew nothing about its action. He was simply dealing with remarks which he had heard in the room that evening and at the previous meeting of the society.

Vet.-Capt. A. J. HASLAM said he rose with considerable diffidence because he was in the position of many gentlemen present that evening, speaking about something of which he had really little experience. But it occurred to him to suggest that, apart from the place of growth and the mode of preparation, the distance over which and the temperature through which the drug travelled before it came to be used in an English laboratory or upon a patient, must have resulted in considerable variation. Independently of this he thought they had somewhat overlooked the fact that Captain Rutherford's experiments with this drug were on patients not suffering from abdominal complaints but were nearly all, for the purposes of the drug, healthy animals. The effects he found were not in agreement with those discovered by the practitioners who had used the drug at home. But it was important to remember the difference of action of a drug in health and in disease. A quarter of an ounce of tincture of opium given to a healthy horse had a stimulating effect, but administered to an animal suffering from colic it produced no such effect. Differences of opinion and of observation might be accounted for by differences of condition in the patients, and also because of the undoubtedly uncertain preparations of the drug obtained. He thought that the diversity, the variation in the results of the use of the drug were so great that they were only explicable on grounds of difference in the quality of the drug itself. He could not think of any drug that had effects so diverse. It was only, in his opinion, by obtaining exact products that they could compare effects. They related their experiences of a drug that differed in quality. They were indebted to their guests that evening for trying to obtain an exact product with which they could compare effects. Dr. Easterfield asked whether in India, Indian hemp was administered for veterinary purposes to any great extent, and doubted whether he would find an active principle having a good effect at home for veterinary purposes, unless Indian experience was in his favour. There was no doubt, however, that Dr. Easterfield and Mr Marshall had obtained an active principle which by-and-bye would be placed on the market and they could then judge of its effects. He could not help thinking with Mr. Hunting how little therapeutics was a science or would be a science until they succeeded in getting their drugs of exact quality. (Applause.)

Dr. EASTERFIELD: May I just make a correction. I said it is doubtful whether from the substance of charas resin a substance can be obtained which has a soporific effect in colic. It is possible that the plant which secrets this excess of resin does not excrete the substance that is present in this gunyah or dried hemp. It is quite possible that the resin secreted by the plant which contains this active principle will produce what somebody politely called *delirium tremens*, though it will not contain the substance that is soporific in colic cases.

Mr. J. A. W. DOLLAR said he had only a few words to add to the discussion and those perhaps did not refer directly to the subject under consideration. But he should be sorry if their medical brethren who had honoured them with their presence that night left the meeting with a wrong impression. One or two speakers, perhaps from inadvertence, had given rise to the idea that colic was a condition which was referable solely to one cause. He desired to point out that the varying results which had followed the use of *cannabis Indica* in certain instances had been more or less due to the fact that when one treated colic one treated a symptom and not a condition, and that what might be very useful in, possibly, cases of abdominal pain due to distention of the bowel might not, perhaps, prove so satisfactory if due to some other condition such as a renal calculus or some other abnormality which sometimes carried all the symptoms that distention of the bowels produced. He must deprecate the attitude of those members who seemed to think that because they had already drugs upon which they could rely, that they should limit their pharmacopœia to them and not endeavour to enlarge it. If they were contented always to stick to the drugs whose action

they thought they understood and refused to accept anything fresh, he was afraid they would at once sacrifice their claim to be a scientific and progressive calling, and he would, therefore, ask those gentlemen, even though they had drugs in whose action they were perfectly satisfied, to experiment with this drug and record their experienced so that, by the collection of results, the veterinary profession might obtain something like a scientific basis on which to found a judgment.

Mr. T. H MANTON said there seemed to be very few gentlemen present who had used this drug for practical purposes. The firm with which he was connected had, in their infirmary, successfully used the drug during the last three or four years. He could give them many instances of the utility of *cannabis Indica*. It was a drug to which he, personally, was very partial, although he admitted there were certainly some disadvantages connected with it. It had been said that there were drugs which they could use which had as good an effect as *cannabis Indica* in relieving pain. He did not agree with the gentlemen who held that opinion. He had seen many cases of so-called "colic" in his infirmary where as much as six grains of morphia was administered every two hours without any permanent relief being obtained. Turning to chloral hydrate he said that four drams was considered a fair dose to start with, and if it were increased to two ounces no doubt that quietened the patent for some time. But in his busy practice the great advantage he derived from the use of *cannabis Indica* was this: if they gave an ordinary horse standing 15 hands four drams, and a cart horse six drams, within about an hour, or an hour-and-half, the veterinary practitioner might safely leave the animals for twenty-five hours and not see them again in that time. It was true they were liable in three hours to turn very cold and sweat a good deal, but, given a good foreman to tend the horses, that did not matter. With regard to the action of the bowels he owned that they got constipation. Concerning the excitement of which Professor Hobday spoke he (Mr. Manton) thought that excitement probably arose in cases when a sufficient dose had not been administered. Six drams produced complete stupor. In all cases in which he had used the drug his belief was that benefit to the animal had resulted.

Dr. JOWETT, speaking of extract of *cannabis Indica*, said his attention was first drawn to cannabin because it was well known that the extract of *cannabis Indica* was one of the most uncertain drugs in the pharmacopœia. He believed that they would find the dose was quarter of a grain and that they should proceed until they got some effect. It was quite clear that they could not do anything with a drug of that description when they did not know what would happen. That was, he believed, one reason why this drug had gone so much out of practice. He saw a paper by Dr. Easterfield mentioning that he had isolated from *cannabis Indica* a definite substance to which he gave the name of cannabinol. He himself had prepared some from the green extract and, like Dr. Easterfield, experienced some difficulty owing to the disagreeableness of the stuff to work. However he derived a substance. That was before the vacation. He did not touch the substances again until about two months ago. He tried the drug upon himself and, unlike Dr. Easterfield, had no visions but simply a profound sleep. Subsequently he took a dose of Dr. Easterfield's preparation, experienced a sensation of sleepiness, and leaning back in his chair had those kaleidoscopic views described by Dr. Easterfield. Brilliant visions came in rapid succession, fading one into the other. In a friend of his the stuff did not produce dreams but sleep. Dr. Jowett went on to say that he took afterwards four grains of extract of *cannabis Indica*. That was at a quarter past six one Sunday evening just after tea. It was not until he was having his supper at a quarter past nine that he felt any effects of the drug. Then a very peculiar sensation came over him, and he could hardly finish supper. He commenced laughing, and lost all sense of time. He had no recollection of what happened that evening. How he got into bed he could not tell. He was informed that he could hardly walk across the room. Dr. Easterfield had an idea which he shared, that very possibly in the extract of *cannabis Indica* there were two active principles. In the first place he had dreams and sleep and no intoxication, but when he again took the extract, and only half one equivalent dose, he became intoxicated. It seemed as though there was something in the cannabinol which produced sleep, and in the extract of *cannabis Indica* some other substance that had an intoxicating effect. He observed no effect of diarrhœa or constipation. On the morning after taking the cannabinol he awoke very happy indeed, but after taking the extract of *cannabis Indica* the effect did not pass off so readily. His mouth was glued together with the thickening of saliva. The conclusion at which he arrived was that cannabinol was not productive of the same effect as extract of *cannabis Indica*.

PROFESSOR PENBERTHY said he rose with the utmost diffidence, but he should like to give his impression of the result of the paper of Captain Rutherford's and the discussion upon it. He was in accord with the view expressed by Dr. Haslam, that there were various parts of therapeutics, and that each part had a great issue that attached to itself. Most of those present were veterinary surgeons desirous of obtaining some knowledge that would prove valuable in the practice of their profession and in the relief of animal suffering. They had received information of the greatest value and heard what was news perhaps to some of them, that there were active principles separated, or probably to be separated, in connection with this drug. They had also heard personal experiences of the physiological effects of the extractum cannabis Indica and cannabinol. They learnt what were the parts of the cerebro-spinal axes or nervous system generally that were influenced by these different active principles. But what they knew from their guests, and could gather from Captain Rutherford's paper, were the physiological effects of the drug. he thought they could not too much insist upon the great difference that existed between the pathological condition and the physiological condition. He was delighted to hear the remarks of Mr. Manton, and he should have been still more pleased to have heard more of the really practical side of the application of cannabis Indica. He had had considerable experience of this drug, and from its uncertainty (which had been emphasised by almost every speaker) he did not find it was much better than any other drug they had at their disposal. But he concurred in the view expressed by his friend Mr Dollar, that they should not stop because they had a drug which satisfied them at present. They were under a great obligation to the gentlemen who had come forward that night to give them the benefit of their experience. Happily they had not yet finished their experiments. The result of their work to him, at any rate, pointed to a pretty clear hope that they would have by and bye a stable compound that they could use with reliance.

The CHAIRMAN, in closing the discussion, said his advice to them was to try the Indian hemp in the form of a tincture from the extract. He had tested it in this way and had a very good result. There were five well-known forms of Indian hemp in India. These were churrus, bhang, garjah, majoon and hashish. He gave them in this order according to the proportion of the resinous compound, active principle, they contained. Churrus was the stronger. They would find it principally in the Bengal presidency. The hemp varied in quality in different districts and he advised them to

communicate with the collector or commissioner of a district in order to obtain a good quality and then have the extract prepared by a respectable European chemist in India.

Dr. EASTERFIELD said he derived his hemp direct from the collector in India.

The CHAIRMAN: But you should get the extract from a European chemist in India, and make your own tincture.

Prof. MACQUEEN: In behalf of the author of the paper which I read I have to thank you for your kind treatment of his communication I believe that when he has an opportunity of reading your remarks he will not only be instructed but amused. The discussion has extended far beyond my expectation, and when Captain Rutherford has the pleasure of reading it, it will be quite a surprise to him. I am sorry time does not permit me to make any contribution to the debate myself.

On the motion of Mr. BARRETT, seconded by Mr. REEKIE, a unanimous vote of thanks was accorded to Captain Rutherford for his able paper.

Mr. J. A. W. DOLLAR, moved, and Mr. ROOTS seconded, a vote of thanks to Drs. Easterfield and Jowett and Mr. Marshall for attending the meeting, and relating their interesting experiences.

This, too, was carried unanimously.

On the motion of Mr. BARRET, seconded by Professor PENBERTHY, thanks were unanimously given to the reader of the paper, Professor Macqueen.

At the instance of Professor PRITCHARD a vote of thanks was accorded the Chairman, and with this the proceedings terminated.

EXTRACTS AND NOTES.

A VETERINARY "ANATOMIST."

From an interesting column of historical and archæological notes in *The Norfolk Chronicle* we take the following. No reference to the subject is to be found in *The Veterinarian* for 1838.

It is interesting to note that as recently as the year 1838 there occurred at Norwich a remarkable case which arose out of a person contracting for the sale of his body after death, although the law recognised no right of property in a dead body. A blacksmith named James Maxey died on July 8th, and on the following day his employer, Mr. George Perowne, a veterinary surgeon, practising in the parish of All Saints', waited upon the widow, and informed her that he would have him buried as "a Christian and a gentleman," and pay the expenses of the funeral. On the 10th Mr. Perowne sent a coffin to the house, and arrangements were made for the burial. Whilst Mrs. Maxey was absent from her house on the 11th Mr. Perowne effected an entrance, removed the coffin and the corpse, and carried them away in his gig. The widow went to Mr. Perowne and demanded her husband's body. Mr. Perowne replied that everything was "all right," and desired that the clergyman and bearers should be informed that the funeral would take place next day. Mrs. Maxey and the bearers accordingly assembled in Mr. Perowne's yard at the appointed time, and found the coffin properly nailed down. With womanly instinct, however, Mrs. Maxey suspected that everything was not "all right," and made persistent demands to see the body. Mr. Perowne became excited, procured a gun, drove the whole party from the yard, and locked the gates. It was alleged by Mrs. Maxey and her friends that while they were standing in the street they heard Perowne open the coffin and remove from it several large stones. When the funeral party were re-admitted and allowed to view the body, which was then in the coffin, they found that it presented the appearance of having been "anatomised." Ultimately, it was taken by force and deposited in the steeple of All Saints' church, and Mrs. Maxey made a complaint to the magistrates. Mr. Perowne, on being summoned to attend the Guildhall, entered the court in an intoxicated state, and informed the magistrates that sixteen years previously he paid Maxey a certain sum for the possession of his body after death; that he had dissected it, and found that the man had died from the same complaint as William IV., namely, ossification of the heart. In order to sober Mr. Perowne, the Bench committed him to the gaol for the night, and on the following morning he was formally charged with stealing the body. The accused contended that, as he had purchased it in the interests of science during the life of the deceased, it was his property. The chairman of the Bench replied that "although they knew him to be a very clever man, if he would keep himself sober, yet he was not in any practice that required a knowledge of the anatomy of human body, and his premises were not licensed for anatomical studies." Mr. Perowne, who asserted that he was a member of St. Bartholomew's Hospital was committed for trial at the Assizes. The only reference made to the case in the Assize report was this: "Mr. Perowne was called upon and discharged."—C.M.

PECULIAR NUISANCE CASE AT KILMARNOCK.

Decision has been given by Sheriff Hall in a case arising through the operation of the Board of Agriculture regulations within an infected swine fever area. Sixty pigs were recently consigned to Stewarton Station from Coventry from Mr. M'Clure, Prestwick Road, Ayr. On their arrival their removal was stopped by the police in consequence of Stewartry Parish being within an infected area, but it was afterwards arranged that John Sim, sen., should remove them to an old stable in High Street, Stewarton, occupied by him, but belonging to Mr. James Love, banker. The inspectors for the County Council then served a notice declaring the building an infected place under the Board of Agriculture order. The tenants in the adjoining buildings lodged a complaint with the sanitary inspector of Stewarton against the pigs being there, as they were causing an offensive smell. The sanitary inspector, after being satisfied that a nuisance existed, applied to John Sim, sen., for removal of the pigs, but he stated that he could not remove them after the building had been declared an infected place. The sanitary inspector then applied to the Local Government Board, who advised that the Police Commissioners of Stewarton, as the Local Authority, should apply to the Sheriff for removal of the nuisance under the Public Health Act, and this was done. After the petition was served, however, in consequence of representations that were made to the Board of Agriculture, an inspector from the Board examined the place last Friday, and granted a licence for the removal of the pigs, which were on the following day taken to Hareshaw Farm. Sheriff Hall appointed a hearing on the application on Monday. An agent for Mr. Love stated that he had not let the building as a piggery, and was not aware of the pigs being there. Mr. Sim, sen., said that he removed the pigs to the stable to save them from being starved at the railway station. Sheriff Hall finds that John Sim, jun., was the author of the nuisance, and decerns against him for £3 3s. of expenses.—*North British Agriculturist.*

The Latest Swine-Fever Order.

At a meeting of the Executive Committee under the Diseases of Animals Act, held at the Court House, Northallerton, on Wednesday, the 30th December, 1896, it was resolved—

"That the Committee having considered the Markets and Fairs Swine Fever Order, 1896, are strongly of opinion that in cases of Swine Fever, county boundaries should be disregarded; that the place in which each outbreak occurs should be an Infected Place; that the compulsory attendance of a Veterinary Inspector at every Market or Fair is of no value and causes unnecessary expense; that such expense should not be charged on the County Funds; and that a copy of this Resolution be sent to the Board of Agriculture, and to every County Council in England and Wales."

ARMY VETERINARY DEPARTMENT.

Vet.-Major J. J. Philips joined for duty at Woolwich on 8th instant.

Vet.-Lieut. R. H. Holmes embarked for a tour of service in India, 12th instant.

CORRESPONDENCE.

WART WORT OR RUSSIAN CANCER CURE (?)

SIR,

I thank those gentlemen who have criticised me, in a friendly manner, for my having committed errors of which I am unconscious. In reply I must first of all condemn their manner of argument by attempting to prove that they are right by saying that I am wrong. Their idea, no doubt, is that two blacks make one white.

Your correspondent, Mr. Mayall, asked through the medium of the issue of December 19th of your valuable journal for information concerning a certain kind of wart wort, to wit the Russian cancer cure, and in answer I replied in the issue of December 26th, in which number Mr. Donald also added his mite; and, as his talents did not jump with mine he judged amiss. I don't profess to be an authority on botany, neither have I the ambition of becoming at some future date an examiner on this, to some, very interesting subject. But having no *magnum opus* at hand I shall, to the best of my ability, endeavour to prove, with my popular authorities, that I am right.

Wart-wort and swallow-wort, like many other vernacular botanical words, are used to indicate many different species of plants of different genera. The first time, according to Nuttall's dictionary (edited by the well-known and no mean naturalist, the late Rev. J. G. Wood). is defined as "a plant having a warty surface," and, by Webster (1869) as "a plant having a warty surface of the genus euphorbia and others. Celandine or swallow wort, according to Nuttall, is "so-called because believed to flower with coming and going of the swallow (Gr. Chelidon, a swallow)." Webster says celandine is "a plant, swallow wort, horned or prickly poppy." Hooper, in his medical dictionary (1820) says: "Chelidonium (from xehidar, the swallow.) It is so named from an opinion that it pointed out as useful for the eyes by swallows, who are said to open the eyes of their young by it, or because it blossoms about the time when swallows appear." The herb and root of this plant, the chelidonium majus, tetterwort, and great celandine, have a faint unpleasant smell, and a bitter, acrid, durable taste which is stronger in the roots than the leaves. They are aperient and diuretic and recommended in icterus, when not accompanied with inflammatory symptoms. The chelidonium should be administered with caution, as it is liable to irritate the stomach and bowels. Of the dried root, from ʒss. to ʒj. is a dose; if the fresh root, infused in water or wine, the dose may be about ʒss. The decoction of the fresh root is used in dropsy, cachexy, and cutaneous complaints. The fresh juice is used to destroy warts and films in the eyes; but for the latter purpose it is diluted with milk. But this is going away from the discussion. Let me point out to Mr. Donald, and his champion Mr. Eggleton, that I said nothing about the euphorbiaceæ or spurge-wart order. No doubt they are right in their idea about the euphorbia helioscopia, but this has nothing to do with the particular wart wort enquired after by Mr. Mayall.

Now a word to Mr. Hurndall. He is in error when he says I attributed certain properties to celandine. I said nothing of the kind, but only quoted Bentley's own words. I am no firm believer in the principles of homœopathy. Only when used in faithcuring or as in cases where placebos are required. Respecting the doubt I have cast against the efficacy of celandine in the cure of cancer, I may say that I am a bit of a sceptic against new remedies until I am convinced they will do what is claimed for them, and I am afraid this cancer cure will, like its predecessors, take a back shelf. Many of the "cures" are, in my humble opinion, the work of diagnostic errors.

In conclusion I think the thanks of your readers are due to Mr. Hurndall for his valuable exposition of the action and uses of this old-fashioned or antiquated, but by no means useless remedy for certain, but up to the present unknown affections.

HENRY GRAY.

EDITORIAL CENSORSHIP.

SIR,

The correspondence in the last two issues of *The Record* on the above subject, having made extensive reference to veterinary societies' reports, I shall be pleased if you will allow me to say a word or two from the point of view of a Secretary to a V.M. Society.

None of your contributors, I should presume, have occupied the position of president or secretary to a Society, or they would realize more acutely the difficulties to be met in keeping them in an amicable and prosperous condition. With the exception of the two or three societies in the densely populated parts of the kingdom, there is scarcely one existing whose secretary has not a difficulty in obtaining a respectable attendance of its members, and as those who are not present look to the report for information as to the business transacted, this part of it is of great interest to them, although to the majority of your readers it may seem to be unnecessarily occupying valuable space.

Eloquence is not the strong point of the veterinary profession, and as even amongst practised orators there is not one man in fifty who at the end of a speech has said exactly what he thinks he has said, or what he intended to say when he got on his legs, it is excusable if the contributor to a discussion at one of our society meetings occasionally considers himself misreported. Speeches are not as a rule reported verbatim for obvious reasons, and our lay reporters in endeavouring to get the sense of what they only dimly understand are very apt to make confusion worse confounded.

When the report is received by the secretary there are always alterations to be made, to demonstrate the sense of what one man has said, or to avoid injuring the tender susceptibilities of another, and after it is published the poor fellow catches it again. The first time he meets A, who made a rambling five minutes speech at the meeting, he is sure to be called over the coals because A. has not been as fully reported as B. who said twice as much in half the time, and is perhaps threatened with the resignation of C. because the name of D. instead of his appeared as seconding a vote of thanks, or the election of a new member.

The Secretary is in the best position to understand what was said, and is responsible to his members for the report, and it would be highly unfair to him were the Editor to put his own construction upon a subject discussed, or to omit details which are important, to the non-attending members at any rate, and on the value of which he is not in a position to form an opinion, unless the meeting should have been fortunate enough to have been graced by his presence.

I trust, Sir, that the criticisms of your correspondents will not have the effect of altering the course you have always adopted in this matter. It would be a great relief to secretaries to be able to throw the blame of deficiencies in their reports upon the Editor's shoulders, but it would not, I fear, add to their care in preparing them, or to the interest they evoked in the members of the Society when they appeared in print.

Nothing mundane is perfect, and the *Veterinary Record* is no exception to the rule, but I am sure the whole profession is indebted to you for the energy you have displayed in establishing and conducting it, and I trust that the request you have made for additional matter will not be unavailing, and that we shall all do our best in the future to assist you in making the *Veterinary Record* of ever increasing interest to our fellow readers.

I do not approve of anonymous correspondence, but must ask on this occasion to be allowed to subscribe myself ——— AN HON. SEC.

PROFESSIONAL ETIQUETTE.

SIR,

I shall be pleased if you will allow me to lay the following facts before the readers of your journal, by way of enlightening them how some members of our profession adopt measures, which, I am sure, must be repugnant to any of us who have the welfare of the profession at heart.

In 1889, I began business on my own account in a yard in town here called the Oxford, with two entrances from different streets, one from each, the centre for the country people, viz., where a great many horses are put up on market days; no veterinary surgeon had before been permanently in the yard, it was a venture on my part; I still occupy that portion of the yard, and will do so until the end of March, 1897.

I have taken larger premises exactly opposite one of the entrances to this yard, in fact, just the other side of the street, and since have endeavoured to transfer my connection to the new premises where I reside, at the same time daily using the premises in the Oxford yard.

The proprietors are piqued at my leaving, and have let another portion of the yard to a firm in town, both M.R.C.V.S., who have another yard here, and have had for some years.

They are not content to wait till I leave, but frequent the yard, and have had their sign painted up now which covers almost half of the hoarding of the yard, (about twenty-five yards in length), and probably would have moved in had I not written intimating, that if they did so I should report them to the Registration Committee for conduct which would have been disgraceful from a professional point of view.

If this is tolerated by the Council, then advertising must take a back seat, indeed.

It is, to my mind, procedure such as this, that tends to lower rather than elevate our social position at all times.

Thanking you in anticipation. Yours faithfully,

Swansea. JAS. MURRAY STEWART, M.R.C.V.S.

A WAIL.

SIR,

Few members who live in the large centres have any conception of the trials and difficulties of a young member, who, driven from the busy haunts of men and horses by the spectre of the auto-motor looming in the distance, has sought the seclusion of a remote country district wherein to struggle to keep his head above water and to uphold the status of his profession.

Perhaps it may interest and perchance amuse them if I relate my experience of the last few days.

At nine p.m. on Saturday last (the fact that it was raining hard and as dark as pitch added zest to the enterprise) I was called to a farm eight miles away to see a bay mare. The owner, who had ridden in, informed me that she had been ill since the morning, that he had called in Mr. Duck (for obvious reasons I substitute that for his own name) and that at 8 p.m. he, Mr. D., had said he could do nothing more. Mr. H. (the owner) then came for me leaving Mr. D. to await my arrival. Having examined the mare I found her as follows: Mucous membrane intensely injected, respiration 60°, pulse imperceptible, temp. 104-5°; rectum, with the exception of a little slimy mucus, empty, bladder enormously distended; the patient continued walking round the box as is the case in twist.

I then called Mr. D. aside and the following conversation took place: "Well, sir, what do you think of her?" His reply came swift and sudden "She has *the* inflammation and is'nt worth a penny." This being so conclusive I thought reply needless. I then enquired "What have you given her?" "Oh! two drenches."—"And they contained?" "Liquids." I then tried another line, "What laxative has she had?" "Oil."—"How much?" "Half a pint."—"Anything else?" "Yes, aloes."—"And how much?" "Seven or eight drachms." Seeing further questions were useless I returned to the box and applied the usual remedies to which she somewhat slightly responded, the catheter especially giving relief.

I then left her for half an hour, and on returning found the door open and the mare wandered into a field, and here, sir, comes I think the cream of the whole proceeding. Mr. D. humanely remarking "Oh, I shouldn't trouble to get her in she is bound to die and might as well die out there as in the box." I do not think this wants enlarging on. I then left giving the owner a very unfavourable prognosis, but was agreeably surprised when on Sunday morning he again came for me, and from that time she has made an uninterrupted recovery, needless to say without the intervention of Mr. Duck.

Now, sir, I do not cite this case as being at all extraordinary, but seeing that we have our Defence Association and prosecutions under the V.S. Act, can nothing be done to stop such a man who is neither qualified nor registered (his father castrated, he followed suit and, therefore, must be capable to doctor) from going about the country trusting in providence and turpentine, to the utter ruin of the farmer, his stock, and such unfortunate members of our noble profession as "BENIGHTED."

[The only thing to do is to spread knowledge amongst stock-owners, show the superiority of the certified man over the uncertified and—wait a generation or two.—ED.]

MR. DONALD AND THE BORDER COUNTIES VETERINARY MEDICAL SOCIETY.

SIR,

My name has been used a good deal in evidence at this Society's last two meetings, and a good deal of misstatement and misrepresentation seems to have been indulged in. If the members had seen fit to quietly accept my resignation without further comment there the matter would have been allowed to rest, but after what has passed I cannot allow the remarks published to go unchallenged, nor the acts of its officials uncriticised.

The facts are as follows:—At the last meeting of 1895, held on November 8th, I gave notice of the following motion for the next meeting: "That this Society discontinue furnishing *in extenso* reports of its proceedings to the press, but that its meetings be open to representa-

tives of the professional journals to take and publish such reports of our proceedings as they may deem necessary." The next meeting which should have been held the second Friday of February, 1896, and at which this motion was due was not for some reason called until March 13th of that year.

Now every one knows the nature of a country practice at this particular season of the year, its demands upon the practitioner's time and the consequent difficulties of leaving same. The day previous to the meeting, forseeing the probable impossibility of leaving business, I wrote the President saying that if possible I would attend the meeting and was quite prepared to go on with the motion standing in my name, but in the event of my not being able to attend I requested him to allow the matter to stand over till the next meeting. I also intimated that I had sent another notice of motion to the secretary *re* the subscription. By the same post I also wrote to the secretary enclosing written notice that at the next meeting I would move that the annual subscription be reduced to its former level.

Now what happened?—On the motion of Mr. Garnett, seconded by Mr. McKie, it was decided not to grant my request that the motion standing in my name be allowed to stand over. Not a single syllable of reason is given in the report for their, what I call, extraordinary action, nor was any intimation sent to me by either the secretary or president, neither did the published report of their proceedings contain any mention of the notice of motion which I had given at this meeting, but I hardly suspected that it had met the same fate as my other motion, but imagine my surprise when the next meeting for September 4th was called and the agenda did not contain the resolution of which I had given notice. Now it seemed beyond doubt that both my resolutions had been similarly disposed of, consequently I at once addressed a letter to the secretary stating "I could no longer remain a member of the Society after the proceedings at last meeting," and added that "perhaps the chairman would explain to the meeting his acceptance of motions which were contrary to all precedent and opposed to established regulations and settled practice." I could not but feel that I had been grossly insulted by the president and the handful of junior satellites who composed the Penrith meeting and acted with him. I submit it to your readers if any man possessing ordinary self-respect could longer remain a member of this Society after such treatment. To this communication the chairman, after indulging in some unfounded and utterly untrue insinuations which are a direct insult to the dignity of the presidential chair, went on to say that he did not understand its meaning, and stoutly challenged anyone to say he had acted contrary to precedent. I accept that challenge, but it is not sought to deny that the meeting in a strictly legal sense had a right to refuse the postponement of my motion if the proceedings of a society like this are to be viewed in the light of the rules of procedure which govern corporations and other important public bodies, or where the matter could be considered urgent; but I most positively assert that it has always been the practice of this Society, and I believe also of other similar associations to treat the request of the proposer in the most liberal spirit and courteously grant his desire. There are parallel cases in the annals of this Society of which these gentlemen must be quite well aware, and they were always so treated. Surely then this was, as I say, acting contrary to precedent, etc., or if it is not I leave it to Mr. Armstrong to say what it is.

In the other case it is even worse, for it is not only contrary to precedent, established regulations, and settled practice, but absolutely beyond the powers of either the chairman or the meeting to strike out a legitimate notice of resolution which is given in strict conformity of their rules, and that without even questioning its legitimacy. These, I think, effectually dispose of Mr. Armstrong's challenge, and it seems a great pity that if he did not understand the ordinary duties of the office that he did not strive to emulate the conduct of his immediate predecessor in office, Mr. Soulsby. As I understand the ordinary functions of a chairman they are essentially ministerial, to preserve a strict impartiality and not only not indulge in, but instantly to suppress any approach to personality by those over whom he presides.

By the way of apparent justification for their conduct it has been stated by Messrs. Garnett and Hewson, and the President that I had brought forward this motion several times. This is not true, and I say most positively and deliberately that neither this nor any similar motion has ever been before a meeting of the Society, and I here openly challenge them to show that it has or that the facts are not substantially as I have stated them above.

Mr. Thompson, the treasurer, is reported to have informed the meeting—quite correctly—that I objected to pay the current subscription on the ground "that I was not in the enjoyment of members' privileges," and went on to say "he could not understand Mr. Donald taking umbrage in this matter. He had had all the privileges enjoyed by other members, (Hear, hear)." Now while I am not at all surprised at the utterances of the three other members named I confess that I was astonished to read that a gentleman of Mr. Thompson's experience of business, and otherwise general fairness, publicly state that a member who had had a perfectly legitimate and orderly notice of resolution refused (either by the officials alone or with the assistance of the meeting) and consigned to the waste paper basket is in the full enjoyment of all privileges of membership. I desire to call his attention to No. 10 of their own rules and ask if in the face of this he still holds the same view.

At their last meeting recorded in your issue of the 2nd inst., the president rose with the avowed intention of making some explanation, but he evidently quite forgot his purpose for beyond making a few groundless insinuations the only information he vouchsafed was that I had been too much patted on the back. The sort of patting on the back which I have received from him and his satellites will be readily gathered from a perusal of the above facts.

True, I am no longer a member of the Society which owes its inception solely to my effort and a measure of its earlier success to my endeavour, but I still cherish an admiration for the amount of good work it has done in the past and best wishes for what it may accomplish in the future. But I cannot close this epistle without remarking on the absence of those old and respected members who in times past were the very backbone of its meetings, but who, alas, are now conspicuous only by their absence, and ask the present officials how much of this lamentable state of affairs is due to their want of courtesy, unbusiness methods, extravagance, and bad management, and I do not hesitate to say that they must feel themselves blameable for it in a very large degree; but I may express the hope sincerely now that Mr. Thompson, with his better knowledge of business and greater civility may be able to bring this moribund Society back once more to its former level of prosperity and usefulness.—Yours truly,

J. DONALD, V.S.

[The functions of an editor are similar to those Mr. Donald allows to a chairman, but they are not always put into action.—ED.]

COMMUNICATIONS, BOOKS, AND PAPERS RECEIVED—Messrs. J. Donald. G. T. Cannon, D. Pugh, W. Brown, J. M. Stewart. "An Hon. Sec.," "Justita non Amicitia."

American Vety. Review (Jan.), *The Newbury Weekly News*, *The Financial Post*, *The South Eastern Gazette*.